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COMMENT

Automaticity of Unconscious Response Inhibition: Comment on Chiu and Aron (2014)

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A recent study (Chiu & Aron, 2014) suggested that unconscious response inhibition is maintained when subliminal stimuli are mixed with supraliminal stimuli that are associated with response inhibition (mixed session), but it is abolished when they are presented alone (single session). However, awareness of the subliminal stimuli is likely to differ in the 2 sessions because of *priming of awareness*—awareness for subliminal stimuli is elevated (e.g., no longer subliminal) when mixed with supraliminal stimuli (Lin & Murray, 2014a). Here, in a novel design, we measured the awareness level in both sessions and found that the session-dependent effect was due to an awareness difference: The effect disappeared when awareness was comparable and emerged only when awareness was different. Arguments based on the lack of correlation between awareness and unconscious effects are refuted because typical correlation analysis underestimates the true correlation because of *range restriction* and it speaks only about individual differences that cannot explain within-subject effects (e.g., stimulus context here). Our findings also point to an attention-based mechanism underlying priming of awareness: Supraliminal trials are less attention-demanding, allowing for more attentional resources for subliminal trials in the mixed than single sessions. We discuss 2 implications. First, unconscious effects depend on top-down task sets and bottom-up stimulus strength. Second, to properly demonstrate unconscious processing, we stress the importance of having equivalent trial sequences between the main and awareness tests, promote a conjunction method that can strengthen inference, and discuss establishing a limit for equivalence between observed and chance performance.

Keywords: priming of awareness, unconscious response inhibition, task set, cognitive control, go/no-go task

Conscious awareness of a stimulus has traditionally been thought to be regulated by two principal factors: the conspicuity of the stimulus and our momentary focus of attention (Dehaene, Changeux, Naccache, Sackur, & Sergent, 2006). However, recent evidence has revealed another important but previously overlooked factor: past experience (Lin & Murray, 2014a). For example, perceptual history from preceding trials can exert a powerful influence on awareness, elevating awareness of low-visibility stimuli when these stimuli are mixed with high-visibility stimuli (*mixed* session) relative to when presented alone (*single* session; Figure 1).

This phenomenon, called *priming of awareness*, is highly robust: It emerges rapidly during the first two (one mixed, one single) sessions in the test and remains strong afterward. One main mechanism through which high-visibility stimuli induce priming of awareness is shape-specific template enhancement (Lin & Murray, 2014a). Consequently, when low-visibility and high-visibility trials are of the same set of shapes—thereby maximizing shape-specific template enhancement—awareness elevation is robust for low-visibility stimuli that are presented either for 16.7 ms or for 33.3 ms (Figure 2, top panel). However, when the low-visibility and high-visibility trials are of different sets of shapes—thereby minimizing shape-specific template enhancement—awareness elevation is confined only to low-visibility stimuli of 33.3 ms, not 16.7 ms (Figure 2, bottom panel; see *Discussion*).

The current article focuses on a main implication of this phenomenon, namely, on proper measures of awareness. This methodological issue sits at the heart of research in unconscious perception and cognition, for the very claim of unconscious effects relies on the stimulus of interest being truly out of awareness (e.g., Lin & He, 2009, Box 1). The dominant approach to unconscious processing uses a dissociation logic, contrasting two distinct tasks that are typically in different sessions: an indirect task, such as priming or adaptation, which probes the potential processing of the invisible stimuli (*main* test); and a direct task, such as forced-choice discrimination,

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Zhicheng Lin conceived, designed, and conducted the research. Zhicheng Lin and Scott O. Murray wrote the manuscript.

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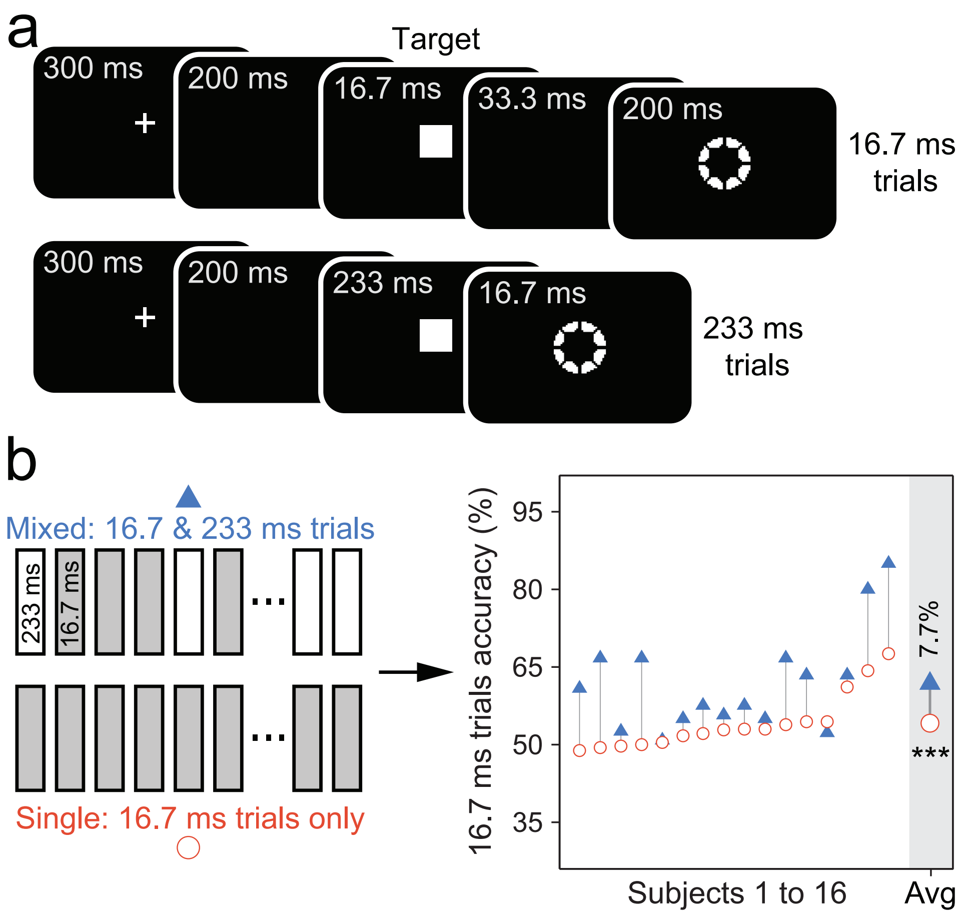

Figure 1. Priming of awareness. (a) Trials: A square or diamond, serving as the target, was presented and then masked; it was presented either for 16.7 ms (thus strongly masked and of low visibility) or for 233 ms (weakly masked and of high visibility). (b) Conditions and awareness results: In the *mixed* blocks, the 16.7-ms trials were randomly mixed with the 233-ms trials, as in the "with executive setting" blocks in Chiu and Aron (2014); in the *single* blocks, only the 16.7-ms trials were presented, as in the "without executive setting" blocks in Chiu and Aron (2014). Each line represents an individual subject (or their average): diamond = mixed condition; circle = single condition. Forced-choice accuracy for the 16.7-ms trials was much higher in the mixed than single blocks (data from Experiment 1B in Lin & Murray, 2014a). *** = $p < .001$; avg = average. See the online article for the color version of this figure.

which tests whether the stimuli are truly invisible (*awareness* test). Finding a positive effect in the main test but a negative effect in the awareness test—hence a dissociation between them—is taken as evidence for unconscious processing. Priming of awareness dictates that the *trial sequence* be equivalent between the two sessions—a principle that echoes the classic notion that the main test and the awareness test should generally be as similar as possible (e.g., Reingold & Merikle, 1988). Therefore, if the main test mixes low-visibility and high-visibility trials, then the awareness test should also mix the two types of trials and vice versa (Lin & Murray, 2014a).

As simple and straightforward as the principle of equivalent trial sequence is, it has not been widely followed in research on unconscious perception and cognition, perhaps because of the implicit belief that violating this principle only has negligible or benign consequences. We seek to test this assumption in a case study, specifically the study by Chiu and Aron (2014), by combining conceptual and empirical analysis.

We start by first describing the basic structure and rationale of their study. There were three sessions: first a session that mixed go trials and no-go trials (*go & no-go trials* session), then a session that consisted only of go trials (*go trials only* session), and at the end an awareness test. The key issue lies with the way the awareness test was structured: Its trial sequence was only equivalent to the go trials only session—and different from the go & no-go trials session. This problem and its associated consequences are corroborated below.

The trials in the go & no-go trials session had two different levels of visibility (Figure 1a): On each trial, either a diamond or a square target was presented and then masked by an annulus, with the duration of the target either very brief (16.7 ms; hence, strongly masked and of low visibility) or long (233 ms; hence, weakly

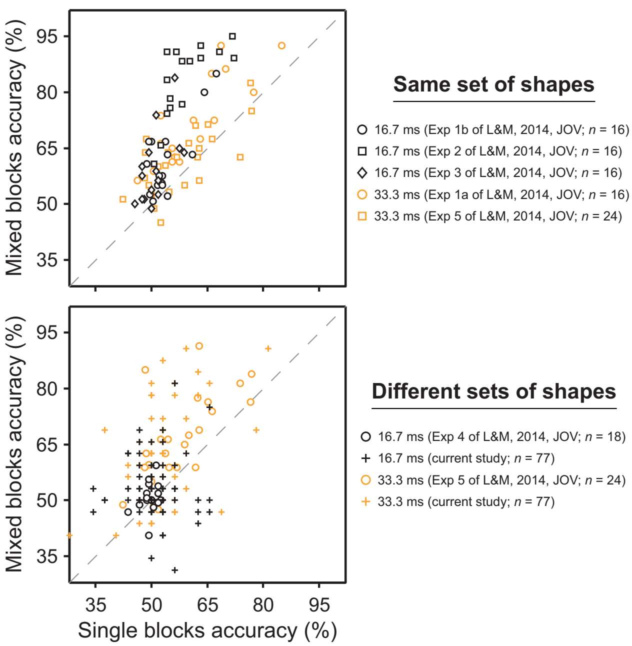

Figure 2. Meta-analysis of priming of awareness based on Lin and Murray (2014a)—referred to as "L&M, 2014, JOV" in this figure—and the current study. The top panel summarizes the data from experiments using the same set of shapes (e.g., both square and diamond) for the low-visibility and high-visibility trials whereas the bottom panel from experiments using different sets of shapes (e.g., as in the current study; see Figure 3). Each data point represents a single participant: Falling above the diagonal dash line is consistent with priming of awareness; below inconsistent with it. Although these experiments differed in several key aspects, including experimental design and task, cautioning against direct comparison of effect size across durations (16.7 ms vs. 33.3 ms) or conditions (same vs. different sets of shapes), two general conclusions emerge: For the same set of shapes, priming of awareness is highly robust and strong for 16.7-ms trials (13.2%, *SEM* = 1.5%) as well as for 33.3-ms trials (6.3%, *SEM* = 1.2%); however, for different sets of shape, the effect was robust only for 33.3 ms trials (7.7%, *SEM* = 1.1%) but minimal and not significant for 16.7-ms trials (1.6%, *SEM* = 0.9%)—a pattern observed in both studies. See the online article for the color version of this figure.

masked and of high visibility). Participants performed a go/no-go task based on the target shape and its visibility: to withhold response (*no-go*) when a visible square appeared, but press a button (*go*) otherwise—that is, when a visible diamond appeared or when the target was invisible. Accordingly one visible shape (square) was associated with no-go whereas the other (diamond) was associated with go. Of interest were the invisible trials: as typical in studies of unconscious inhibitory control (van Gaal, Ridderinkhof, Scholte, & Lamme, 2010; but see Lin & Murray, 2014a), participants were found to respond more slowly when the invisible target was a square than when it was a diamond.

Moving one step further, Chiu and Aron (2014) asked the *necessary* condition of this so-called unconscious stopping effect: After the go/no-go task set is established (by means of the visible trials in the first session), does the manifestation of the unconscious effect depend on conscious stopping (i.e., the invisible trials still being mixed with visible, no-go trials)? They reasoned that (a) no-go trials were still necessary for maintaining a state of preparing for inhibiting a response—a response inhibition executive setting, in their terminology, and that (b) this executive setting was necessary for unconscious stopping to occur. Therefore no-go trials were hypothesized to be necessary for the unconscious stopping effect. This hypothesis was tested in the second session, the go trials only session, which consisted of the invisible trials from the first session but without the visible trials; hence, this session required only a go response for each trial. The prediction was that if no-go trials were necessary for the unconscious stopping effect, then there should be an interaction effect between invisible stimuli (diamond vs. square) and sessions (go & no-go trials session vs. go trials only session).

After these two sessions, to ensure that the low-visibility trials were indeed invisible, in the final session Chiu and Aron (2014) measured the accuracy for forced-choice shape discrimination. This awareness test comprised only the low-visibility trials without the high-visibility trials—making the trial sequence similar to the go trials only session but different from the go & no-go trials session. The critical results and their interpretation are as follows. First, accuracy in the final awareness test was 51.9%, not significantly different from chance; more critical, as predicted, was a significant interaction effect in response times between invisible stimuli (diamond vs. square) and the first two sessions (go & no-go trials session vs. go trials only session)—the effect of invisible stimuli (i.e., unconscious stopping) was significant in the go & no-go trials session but not in the go trials only session. These results led them to conclude that no-go trials were indeed necessary for the unconscious stopping effect to occur.

However, this conclusion is premature because proper interpretation of the interaction effect requires the awareness level to be comparable between the two different sessions, an assumption not tested in the original study—recall that the awareness test used a trial sequence that paralleled the go trials only session but differed from the go & no-go trials session. In fact, a direct test of this assumption—by measuring awareness in both the go & no-go trials session and the go trials only session using the same stimulus parameters as in Chiu and Aron (2014)—revealed a striking difference: Awareness of the low-visibility trials was much higher in the go & no-go trials session (mixed) than in the go trials only session (single), an instance of priming of awareness (Lin & Murray, 2014a; see Figure 1b). Such a difference in awareness challenges the conclusion drawn by Chiu and Aron (2014) in two ways: First, the ostensibly unconscious inhibition effect in the go & no-go trials session may have been a conscious effect; second, the interaction between invisible stimuli and sessions was confounded by an intrinsic difference in visual awareness between the two sessions.

Nevertheless, just demonstrating an awareness confound in Chiu and Aron (2014), as we do above (Lin & Murray, 2014a), does not necessarily mean that their conclusion is false. For example, it could be that, after excluding this awareness confound, the same results as in Chiu and Aron (2014) still hold—or not. Therefore, it remains unknown whether executive setting—as conferred by performing occasional outright response inhibition—is necessary for unconscious response inhibition. Following the logic of Chiu and Aron (2014), the specific key question then is this: Does the interaction effect between invisible stimuli and sessions hold when awareness is correctly measured and equated in both sessions?

This intriguing question eludes the conceptual analysis above; therefore, we decided to empirically investigate it.

As discussed above, the central approach is to compare the unconscious inhibition effect between two different types of sessions: one mixing low-visibility trials and high-visibility trials (i.e., the mixed, go & no-go trials session) and the other including only low-visibility trials (i.e., the single, go trials only session). In traditional designs in which the low-visibility and high-visibility trials are of the same set of shapes, because of priming of awareness, awareness of the low-visibility stimuli is higher in the mixed session than in the single session (Figure 2, top panel). This intrinsic difference poses a significant challenge for directly comparing the two sessions.

In this study, we were able to circumvent this issue by exploiting two recent findings. First, when shape-specific template enhancement is minimized, by using different sets of shapes for low-visibility and high-visibility trials, awareness elevation is abolished for low-visibility stimuli of 16.7 ms (Lin & Murray, 2014a; see Figure 2, bottom panel). Second, the abstract, same–different relation between objects can be processed without awareness even when the specific shape exemplars vary across trials (Lin & Murray, 2014b). Integrating these two findings was a revised design illustrated in Figure 3. Each trial now included two target objects that could be the same or different, and critically the set of objects for the low-visibility trials was distinct from that for the high-visibility trials. The go/no-go rule was based on the shape relations (same vs. different). To test the generalizability, we also manipulated the duration of the low-visibility stimulus (16.7 ms vs. 33.3 ms)—which, as a manipulation of stimulus strength, has been shown to modulate the magnitude of the unconscious stopping effect (Lin & Murray, 2014b).

On the basis of previous research (Lin & Murray, 2014b), we expected participants to respond more slowly when the invisible shapes were different than when they were the same because of the go/no-go task set established by the visible trials (i.e., "different" being associated with no-go but "same" being associated with go). The critical question was whether this effect of invisible stimuli (different vs. same) depended on whether they were in the go & no-go trials session or in the go trials only session—in other words, whether there was a stimuli × sessions interaction effect.

Method

Participants and Apparatus

The effect size for the interaction effect in Experiment 2 of Chiu and Aron (2014) was $\eta_p^2 = 0.40$ (Y. C. Chiu, personal communication, August 6, 2014). With $\alpha = .05$, detecting such an effect with 99.9% power requires a total sample size of 41, 99.0% power requires $n = 30$, and 80% power requires $n = 15$ (G*Power 3.1; Faul, Erdfelder, Lang, & Buchner, 2007). Here, the number of participants was predetermined to be four times of that used by Chiu and Aron (2014, Experiment 2; $n = 20$). Accordingly, 80 college students (46 female, 34 male; age: $M = 19.3$ years; $SD = 1.7$) with normal or corrected-to-normal vision participated. The experiment was conducted in accordance with and approved by the Institutional Review Board of the University of Washington.

The stimuli were presented on a black-framed 21-inch CRT monitor (Sony G520 at 60 Hz and 1024 × 768 pixels). Participants sat approximately 80 cm from the monitor with their heads positioned in a chin rest in an almost dark room.

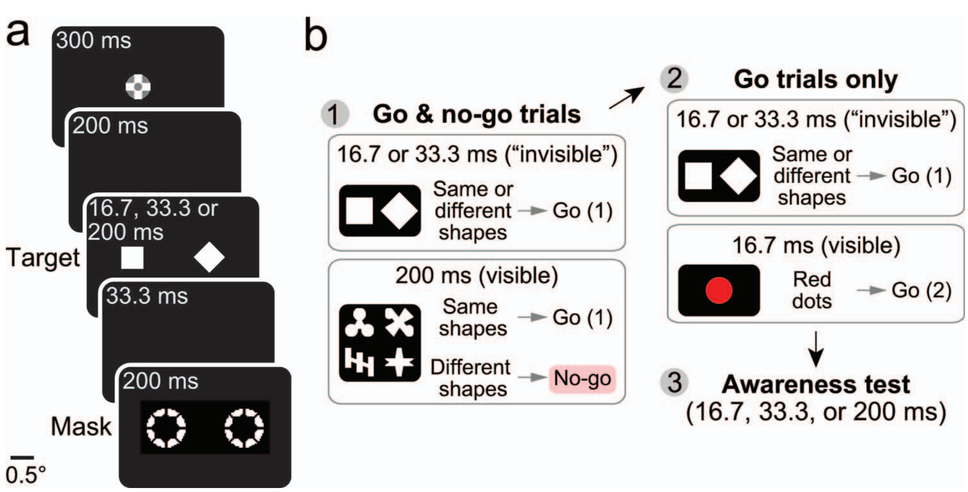

Figure 3. Method. (a) Each trial started with a central fixation, then two target objects, followed by two mask annuli. The duration of the two objects could be short ("invisible") or long (visible), and they could be of the same shape or of different shapes. (b) There were three main sessions: two go/no-go sessions and an awareness-test session. In the first go/no-go session, the task was to press a button as quickly as possible on each trial unless the shapes were visible and different (i.e., the concept of "different" shapes was associated with no-go). The second go/no-go session was the same except that all trials were invisible (i.e., all were go trials); to ensure that participants paid attention to the target objects, occasionally the target was two clearly visible red dots, which commanded a different key press. After the go/no-go sessions, participants went through an awareness test to discriminate whether two target objects on each trial were the same shape or not. See the online article for the color version of this figure.

Structure of the Experiment

The experiment consisted of two main phases: a go/no-go task and an awareness test.

The go/no-go task. After fixation training (as in, e.g., Lin & Murray, 2013, 2014a, 2014b), participants took part in two different sessions of a go/no-go task. The first session included no-go trials (i.e., requiring an executive setting; 80 trials $\times$ 6 blocks $=$ 480 trials), whereas the second session did not (i.e., without an executive setting; 80 trials $\times$ 3 blocks $=$ 240 trials).

Figure 3a illustrates each trial in this go/no-go task. A central fixation mark was first presented for 300 ms against a black background (luminance $=$ 0.1 cd/m^2), followed by a blank screen for 200 ms. Two target objects (luminance $=$ 106 cd/m^2; contour-to-contour distance $=$ 0.80°) were then presented for 16.7 ms (25% of trials), 33.3 ms (25% of trials), or 200 ms (50% of trials). After a 33.3-ms blank, the target objects were masked by two annuli lasting 200 ms (luminance $=$ 106 cd/m^2; diameter $=$ 0.80°) with a 1,200-ms blank at the end. The fixation mark was a combination of a bulls eye and cross hairs (diameter of inner circle $=$ 0.16°; diameter of outer circle $=$ 0.50°; luminance $=$ 106 cd/m^2 for cross hairs and 24.4 cd/m^2 for bulls eye), which the participants had to fixate. When the two target objects were presented for 16.7 ms or 33.3 ms (and thus were strongly masked and "invisible" to the participants), they were selected from square (size $=$ 0.47°) and diamond (size $=$ 0.47°) shapes; when they were presented for 200 ms (and thus were visible to the participants), they were selected from four novel shapes (size $=$ 0.47° $\times$ 0.47°; see Figure 3b).

The task in the go & no-go trials session was to press a key ("Enter")—referred to as go (1) in Figure 3b—as quickly as possible if the masks were preceded by two visible same shapes (go; 25% of trials) or by two invisible shapes (go; 50% of trials, equally likely to be same or different), but to withhold response if the annuli were preceded by two visible different shapes (no-go; 25% of trials). Thus, visible pairs of *different* shapes were associated with *no-go* whereas visible pairs of *same* shapes were associated with *go*. To counterbalance this stimulus–response association, half of the participants were instructed the other way around: to associate *same* with *no-go, different* with *go*. In the article, for the sake of simplicity, only the former association was used as illustration when describing the procedure. Participants practiced for a random sample of 16 trials before proceeding to the formal session.

The task in the go trials only session was the same except for the omission of the visible trials; that is, all of the trials were go trials. The two target shapes were presented for 16.7 ms (40% of trials) or 33.3 ms (40% of trials). As in Chiu and Aron (2014, Experiment 2), to ensure that participants did attend to the target, on 20% of trials the target was two red dots presented for 16.7 ms; upon detecting this target, participants were asked to press a different key ("F") as quickly as possible—referred to as go (2) in Figure 3b.

The awareness test. Immediately after completing the go/no-go task, participants went through an awareness test to discriminate the shape relation for a total of 192 trials (after 8 practice trials). In the first 128 trials, the shapes were presented for 16.7 ms (25% of trials), 33.3 ms (25% of trials), or 200 ms (50% of trials), as in the go & no-go trials session; in the final 64 trials, the shapes were presented for 16.7 ms (50% of trials) or 33.3 ms (50% of trials), as in the go trials only session. The task was to indicate whether the two target shapes were the same or different. Participants were informed that "response time is not important" and were asked to "respond as accurately as possible." Each trial ended as soon as 1,200 ms after the offset of the masks or until response, whichever was later. Other aspects were the same as the go/no-go task.

Data Analysis

As Figure 3b illustrates, the invisible-target trials required go responses; reaction time (RT) in these trials was the key measurement. There were three factors in this design: (a) *stimuli*, whether the target in the invisible trials consisted of two different shapes or of two same shapes; (b) *sessions*, whether the invisible trials were in the go & no-go trials session or in the go trials only session; and (c) *durations*, whether the target in the invisible trials was presented for 16.7 ms or for 33.3 ms.

Because the participants who went into the data analyses for the 16.7- and 33.3-ms conditions were not all the same—some entered the 16.7-ms condition but not the 33.3-ms condition and vice versa (see *Results and Discussion*)—a full factorial analysis was not feasible. Instead, given that the critical question concerned whether the effect of stimuli—faster RT when the stimuli were the same than different (Lin & Murray, 2014b)—depended on whether these invisible trials were in the go & no-go trials session or in the go trials only session, this question would be adequately addressed by examining whether a significant stimuli $\times$ sessions interaction effect emerged in either duration.

Before we conducted these analyses, we preprocessed the data as follows. In the main test, in keeping with previous studies (Chiu & Aron, 2014; van Gaal et al., 2010), trials were excluded if the RT (calculated from the onset of the annuli to the time of response) was faster than 100 ms or slower than 1,000 ms (0.36% of trials). In the awareness test, as recommended in Lin and Murray (2014a), three participants were excluded because of low accuracy in the visible trials (accuracy was 59.4% for two and 65.6% for another, not significantly above chance; for comparison, other participants averaged 94.7% correct, ranging from 75.0% to 100%).

Results and Discussion

We first examined forced-choice accuracy in the awareness test, which included two trial sequences as used in the mixed and single blocks. A repeated measure analysis of variance (ANOVA) on trial duration (16.7 ms vs. 33.3 ms) and block type (mixed vs. single) revealed not only their main effects, $F(1, 76) = 20.24$, $p < .001$, $\eta_p^2 = 0.21$ and $F(1, 76) = 26.26$, $p < .001$, $\eta_p^2 = 0.25$, respectively, but also their interaction effect, $F(1, 76) = 11.99$, $p < .001$, $\eta_p^2 = 0.14$. In other words, as Figure 4 shows, higher accuracy in the mixed than single blocks emerged only for the 33.3-ms trials, $t(76) = 5.72$, $p < .001$, $d = 0.65$, but not for the 16.7-ms trials, $t(76) = 1.64$, $p = .106$, $d = 0.19$. This group-level pattern is overall consistent with a recent study on priming of awareness (Lin & Murray, 2014a, Experiments 4 and 5; see *Discussion*).

At the individual level, a given participant's awareness of the target objects in the go/no-go task was indexed by his or her average forced-choice accuracy in the awareness test. As in Chiu

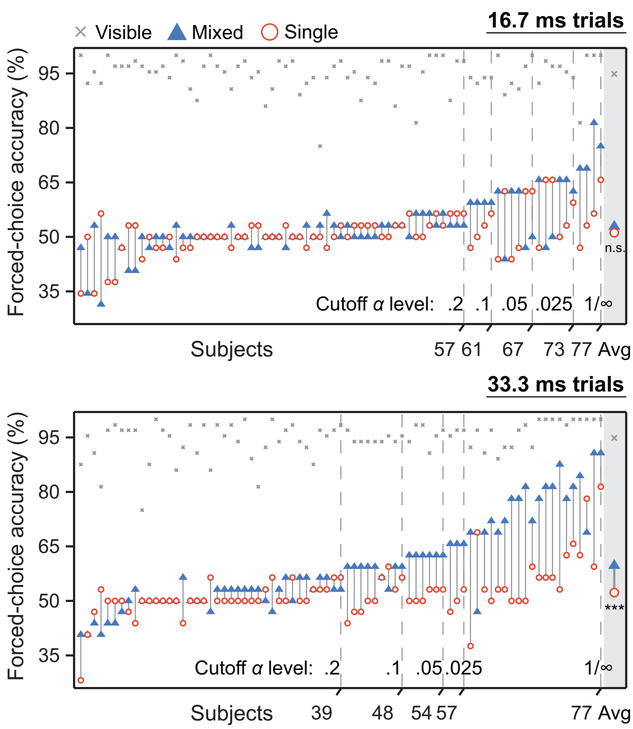

Figure 4. Results of the awareness test. The awareness test consisted of two different forced-choice sessions: one mixing visible (200-ms) and invisible (16.7-ms and 33.3-ms) trials as in the go & no-go trials blocks (i.e., mixed blocks), the other comprising only invisible trials as in the go trials only blocks (i.e., single blocks). For the 16.7-ms trials, forced-choice accuracy did not differ between the mixed and single blocks (top panel), but for the 33.3-ms trials accuracy was higher in the mixed than single blocks (bottom panel). Each dashed line represents an α level (one-tailed) for determining chance versus above-chance accuracy using the binomial test; from left (more conservative) to right (more liberal), the α levels are .2, .1, .05, .025, and 1/∞ (i.e., including all of the trials). See the online article for the color version of this figure.

and Aron (2014), the decision for chance versus above-chance accuracy (i.e., unaware vs. aware) was based on null hypothesis significance testing, specifically the binomial test, with the α level set at .05. Here, to further evaluate the robustness of our results, we also adopted four additional levels, thus ranging from more conservative to more liberal criteria, .2, .1, .05, .025, and 1/∞ (i.e., including all of the trials; see Figure 4).

Crucially, because the awareness level was comparable between the mixed and single blocks, the 16.7-ms trials afforded us to address the critical issue set up in the introduction: whether the effect of invisible stimuli (different vs. same) interacted with sessions (go & no-go trials session vs. go trials only session). For the 33.3-ms trials, the overall awareness level was higher in the mixed than single blocks, but their difference was smaller at more conservative α levels (e.g., .2 and .1) than at more liberal levels (e.g., .025 and 1/∞). Thus, the 33.3-ms trials allowed us to track how the potential interaction effect varied as the degree of awareness difference between the two sessions changed across these different α levels.

Table 1 and Figure 5 summarize the results of the *invisible* trials in the go & no-go trials session and the go trials only session (see Figure 3b). For the 16.7-ms trials, at each and every α level, the awareness level was comparable between the mixed and single sessions. At the same time, the main effect of stimuli (same vs. different) was significant in all of the cases, suggesting that participants responded more slowly when the shapes were different than when they were the same—an unconscious inhibition effect. Critically, in no case did this unconscious inhibition effect interact with sessions (go & no-go trials session vs. go trials only session). In fact, the interaction effect size was consistently small across the five α levels, approximately 0.01—below what is typically considered a small effect, which is 0.02 (Cohen, 1988). For the 33.3-ms trials, at the most conservative α level, the pattern was the same as those for the 16.7-ms trials—no difference between the mixed and single sessions and no interaction effect. However, at the other four (more liberal) α levels, accuracy was significantly higher for the mixed than single sessions, and with it emerged the interaction effect between stimuli and sessions (highlighted in bold in Table 1 and by shaded region in Figure 5). Consistent with these results, at the most liberal criterion (α = 1/∞; see Figure 5, rightmost columns), in which all of the subjects were included in the analysis, thereby permitting a full three-way factorial analysis, the stimuli × sessions interaction was found to depend on the duration (i.e., whether there was an awareness difference between sessions), $F(1, 76) = 7.48$, $p = .008$, $\eta_p^2 = 0.09$.

These results from the 16.7- and 33.3-ms trials converge to provide strong evidence that whether the unconscious inhibition effect (i.e., the main effect of stimuli) interacts with sessions depends on the awareness difference between the mixed and single sessions. When the two sessions have comparable awareness levels, the unconscious inhibition effect does not interact with sessions; when the two sessions differ in awareness, the interaction effect emerges. This finding helps to explain the larger interaction effects in Chiu and Aron (2014): 0.34 and 0.40 for their Experiments 1 and 2, respectively (Chiu, personal communication), compared with ≤0.20 for the 33.3-ms trials here (Table 1). In other words, the awareness difference between the mixed and single sessions in Chiu and Aron (2014) was likely to be bigger than the 33.3-ms trials here because the low-visibility and high-visibility trials were of the same set of shapes in Chiu and Aron (2014), but of different sets of shapes here (Lin & Murray, 2014a, Experiment 5).

We next examined the overall performance on the *visible* trials in the go & no-go trials session and the go trials only session. In the go & no-go trials session, the visible trials included go trials and no-go trials (see Figure 3b); as expected (Chiu & Aron, 2014; Lin & Murray, 2014a, 2014b; van Gaal et al., 2010), participants ($n = 77$) did well on this go/no-go task: for the go trials, responses were fast and accurate (RTs: $M = 266.8$ ms, $SD = 77.2$ ms; miss rates: $M = 2.2\%$, $SD = 1.9\%$); for the no-go trials, the inhibition rate was high ($M = 84.0\%$, $SD = 12.4\%$). In the go trials only session, the visible trials included only the red-dot trials (Figure 3b); responses were fast ($M = 341.0$ ms, $SD = 54.4$ ms), with a high hit rate ($M = 88.7\%$, $SD = 9.2\%$) and a low false alarm (FA) rate ($M = 0.5\%$, $SD = 0.6\%$).

However, two things should be noted. First, the participants who were included in the data analyses were not the same for the 16.7- and 33.3-ms conditions; some entered the 16.7-ms condition but not the 33.3-ms condition and vice versa. Second, more partici-

Table 1
The Results of the Awareness Test and the Main Test for the Two Trial Durations (16.7 ms and 33.3 ms) at Each of the Five α Levels

		Awareness test		Main test ANOVA p value	
Duration	α	Mixed − Single, in %	p value	Stimuli × Sessions (η_p^2)	Stimuli (η_p^2)
16.7 ms:					
	.2	50.1 − 49.8 = 0.3	.772	p = .402 (0.01)	p = .007 (0.12)
	.1	50.7 − 49.9 = 0.8	.401	p = .356 (0.01)	p = .006 (0.12)
	.05	51.0 − 50.2 = 0.8	.446	p = .479 (0.01)	p = .005 (0.11)
	.025	51.7 − 50.8 = 0.9	.383	p = .302 (0.01)	p = .004 (0.11)
	1/∞	52.8 − 51.0 = 1.8	.106	p = .310 (0.01)	p = .005 (0.10)
33.3 ms:					
	.2	50.5 − 49.9 = 0.6	.523	p = .168 (0.05)	p < .001 (0.47)
	.1	52.0 − 50.2 = 1.8	**.055**	***p* = .012 (0.13)**	p < .001 (0.53)
	.05	53.1 − 50.3 = 2.8	**.003**	***p* = .003 (0.15)**	p < .001 (0.53)
	.025	53.8 − 50.3 = 3.5	**<.001**	***p* = .001 (0.16)**	p < .001 (0.53)
	1/∞	59.7 − 52.3 = 7.4	**<.001**	***p* < .001 (0.20)**	p < .001 (0.53)

Note. Percentage correct (PC) can be converted to d' using the formula $Z(PC) - Z(1 - PC)$; therefore, 50.5% corresponds to d' of 0.0251, 51% to d' of 0.0501, etc. The bold values indicate significant effects.

pants were excluded in the 33.3-ms condition than in the 16.7-ms condition whether α was set at .2, .1, .05, or .025. This is expected given that 33.3-ms stimuli are stronger than 16.7-ms stimuli—and it is consistent with the trial duration main effect in the awareness test.

Therefore, to check whether (and how) participants who were excluded from the main data analyses differed from those who entered the main data analyses (see Table 1), we compared those who performed above chance ($p < \alpha$) and those who did not ($p \geq \alpha$) in the awareness test by looking at every α level within each of the two durations (16.7 ms: Table 2; 33.3 ms: Table 3). Consistent across all of these analyses, there was no significant difference between the two awareness groups in any of the six measures in Table 2 (two-sample t tests: t ranged from 0.015 to 1.930, with corresponding p from 0.988 to 0.057, without correcting for multiple comparisons) and Table 3 (t ranged from 0.027 to 1.585, with corresponding p from 0.978 to 0.117, without correcting for multiple comparisons). The similarity between the aware and unaware participants in these six measures is consistent with the fact that whereas these two groups differed in awareness sensitivity, these measures were based on responses to high-visibility trials.

General Discussion

This paper makes two major contributions. The first contribution concerns the role of executive setting—as conferred by performing occasional outright response inhibition—in unconscious response inhibition. On a conceptual level, we noted that interpreting a session-based interaction effect requires comparable awareness levels between the two sessions, but this requirement was violated in Chiu and Aron (2014) because of priming of awareness (Lin & Murray, 2014a). On an empirical level, we showed that when the awareness levels in the two sessions were both measured, the session-based interaction effect was found to be highly dependent on their awareness difference—the interaction effect disappeared when the two sessions had comparable awareness levels—providing novel evidence against the idea that performing occasional outright response inhibition is necessary for unconscious response inhibition.

The second contribution of this paper concerns the mechanisms responsible for priming of awareness. In the original paper on priming of awareness, the primary mechanism uncovered was shape-specific template enhancement (Lin & Murray, 2014a). In addition, it was found that when the high-visibility target set and the low-visibility target set were different, there was no priming of awareness in one experiment (Experiment 4), but a significant effect in another (Experiment 5). This discrepancy is puzzling and difficult to resolve because of the many differences between the two experiments (stimulus duration, experimental procedure, task complexity, etc.). Here, by varying the stimulus duration in the awareness test, we are able to demonstrate that stimulus duration is responsible for this discrepancy: Priming of awareness occurs for the 33.3-ms trials but not for the 16.7-ms trials (see Figure 2 and Figure 4). The observation that priming of awareness occurs for the 33.3-ms trials when shape-specific template enhancement is minimal implies an additional mechanism. One such mechanism is based on the notion that high-visibility trials are easier, demanding less attentional resource, and thereby sparing more attentional resource for performing the low-visibility trials in the mixed block than in the single block. Whether performance in the low-visibility trials benefits from this spared attention resource depends on how difficult the low-visibility trials are—conceivably, trials that are too low in visibility would be beyond redemption.

On Correlating Unconscious Effects With Awareness Indices

At first glance, the finding that the session-based interaction effect was dependent on the awareness difference between sessions appears to be at odds with a correlation result reported in Chiu and Aron (2014). There, no significant correlation was found between the stopping effect (i.e., the main effect of stimuli) in the main test and accuracy in the awareness test—seemingly to suggest that the main effect had nothing to do with awareness. However, this inference is unwarranted because the correlation analysis neither informs the true correlation nor speaks to the main issue here:

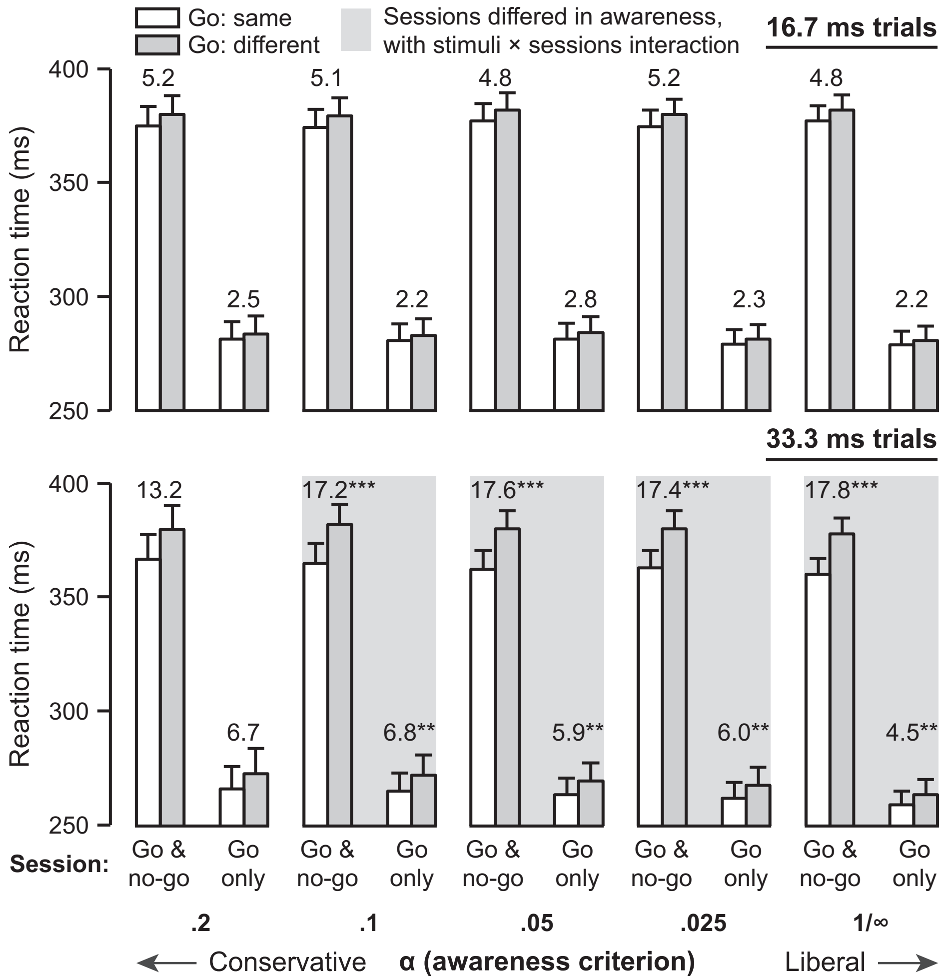

Figure 5. Results of the invisible trials in the go & no-go trials session and the go trials only session. The main effect of stimuli (same vs. different) was significant in all of the cases, showing that participants responded more slowly when the shapes were different than when they were the same. This stimuli effect did not interact with sessions when the two sessions had comparable awareness levels (unshaded region), but it interacted with sessions when the two sessions differed in awareness (shaded region). In other words, the stimuli effect (represented by the value above each pair of columns) was significantly larger in the go & no-go trials session than in the go trials only session when, and only when, the awareness level was significantly higher in the former session than in the latter session. Significant interaction effects were qualified by simple effects analysis (i.e., the stimuli effect in each session), in which ** $= p < .01$; *** $= p < .001$. Error bars show standard errors of the mean.

1. The correlation analysis as conducted is an observed sample correlation that underestimates the true correlation because of *range restriction*. Specifically, by excluding participants who performed above chance in the awareness test, the range of the awareness level among the selected participants, by necessity, is restricted (e.g., both experiments in Chiu & Aron [2014] excluded two participants in this way). Restriction of range is well documented to lead to reduced correlation in statistical principles and in real-world settings (e.g., Pearson, 1903; Sackett & Yang, 2000). For an extreme case, consider the correlation between the main effect and awareness in a sample in which only participants who have the lowest accuracy in the awareness test are selected: The correlation would necessarily be null. This range-restriction account can also parsimoniously explain why a positive correlation between unconscious inhibition and awareness was observed in Lin and Murray (2014a, Experiments 1A and 1B) but apparently not in van Gaal et al. (2010)—range was not restricted in the former but it was in the latter.

2. The correlation analysis is fundamentally about *individual differences*—whether individual awareness correlates with the main effect—whereas at issue here is

Table 2
Performance on the Visible Trials in the Go & No-Go Trials Session and the Go Trials Only Session, Separated for Those Who Performed Above Chance ($p < \alpha$) and Those Whose Did Not ($p \geq \alpha$), With the Criterion for Chance Determined by Accuracy in the 16.7-ms Trials in the Awareness Test

	$\alpha = .2$		$\alpha = .1$		$\alpha = .05$		$\alpha = .025$	
Sessions/Measures	$p < \alpha$	$p \geq \alpha$	$p < \alpha$	$p \geq \alpha$	$p < \alpha$	$p \geq \alpha$	$p < \alpha$	$p \geq \alpha$
Go & no-go								
Go: RT (ms)	269.2	265.9	278.2	263.8	264.1	267.2	331.6	263.2
Go: Miss (%)	1.5	2.4	1.3	2.4	1.5	2.3	1.6	2.2
No-go: FA (%)	15.8	16.1	14.2	16.5	15.6	16.1	8.8	16.4
Go only								
Dot: RT (ms)	322.1	347.7	326.2	344.9	322.9	343.7	340.6	341.1
Dot: Miss (%)	13.6	10.4	13.7	10.6	14.2	10.8	12.5	11.2
FA (%)	0.6	0.4	0.6	0.4	0.4	0.5	0.5	0.5

> the awareness difference between sessions, a *within-subject* effect driven by the stimulus context. In other words, an inference cannot be made on within-subject effects just based on between-subject effects because variance within individuals and variance between individuals can arise from distinct sources. In fact, awareness has been shown to predict priming within, but not between, subjects (Boy & Sumner, 2014).

What the empirical data observed here show is that awareness is strongly linked to the stopping effect. However, this is not to say that awareness is necessary for the stopping effect to occur. On the contrary, the observation that the main effect of invisible stimuli was significant across the board suggests that awareness may not be necessary (Table 1). A more complete interpretation then is that the stopping effect may occur without awareness, but it does increase with the awareness level (see Figure 5).

On What Determine Unconscious Effects

Returning to the question regarding the necessary condition of the unconscious stopping effect, although the current results do not specify the boundary conditions, they nevertheless have implications on a broader question: What may determine unconscious effects in general? By implicating on this broader question, as will be shown below, these results in turn offer important clues to the necessary-condition question.

A major factor that may determine unconscious effects is task sets: the specific task rules that participants internalize during a given experimental setting. This notion has been supported in previous research (e.g., Kiefer & Martens, 2010; Kunde, Kiesel, & Hoffmann, 2003), and it is obvious that the same must also be true here. A unconscious stopping effect is logically impossible without a task set that binds a stimulus configuration (i.e., a pair of different shapes here) to a stopping response in the first place; without it, there is no reason to expect a RT difference between the two types of invisible stimuli (same vs. different)—and hence no unconscious stopping effect. What the current results show is that, contrary to Chiu and Aron (2014), once established, such a task set can persist for at least 240 trials without a significant drop in the effect even when no reinforcement is provided (i.e., without repeating the no-go trials). This is an important result that raises several questions awaiting future research: how far the limit can be pushed and whether (and when) there is a gradual decay in the effect (e.g., Verbruggen & Logan, 2008). It seems reasonable to suggest that moving from a mixed go/no-go session to a go-only session may involve relearning a new task set; therefore, these questions are deeply connected to how changes in task sets influence the way un-

Table 3
Performance on the Visible Trials in the Go & No-Go Trials Session and the Go Trials Only Session, Separated for Those Who Performed Above Chance ($p < \alpha$) and Those Whose Did Not ($p \geq \alpha$), With the Criterion for Chance Determined by Accuracy in the 33.3-ms Trials in the Awareness Test

	$\alpha = .2$		$\alpha = .1$		$\alpha = .05$		$\alpha = .025$	
Sessions/Measures	$p < \alpha$	$p \geq \alpha$	$p < \alpha$	$p \geq \alpha$	$p < \alpha$	$p \geq \alpha$	$p < \alpha$	$p \geq \alpha$
Go & no-go								
Go: RT (ms)	263.4	270.0	268.2	265.9	276.5	262.6	274.7	264.0
Go: Miss (%)	1.9	2.4	2.0	2.3	1.9	2.3	1.8	2.3
No-go: FA (%)	16.6	15.5	16.8	15.5	16.6	15.7	17.5	15.5
Go only								
Dot: RT (ms)	331.1	350.7	330.6	347.3	333.7	344.1	331.7	344.3
Dot: Miss (%)	11.3	11.2	12.1	10.7	12.4	10.8	12.3	10.9
FA (%)	0.4	0.5	0.4	0.5	0.5	0.5	0.5	0.5

conscious information is processed (Kiesel et al., 2010; Schneider & Logan, 2007).

In addition to task sets, another obvious, but much less discussed, factor that may determine unconscious effects is stimulus strength. Although many studies seem to merely dichotomize subliminal and supraliminal processing as if all subliminal stimulus effects should be considered equivalent, as a class ("unconscious"), the data from Table 1 reveal the importance of taking stimulus strength into account when considering the magnitude of unconscious processing. For instance, in Table 1, consider $\alpha = .2$: With similar awareness levels, the effect size for the stimuli main effect was 0.47 for the 33.3-ms trials but only 0.12 for the 16.7-ms trials—a difference between large (>0.26) and medium (>0.13) effect. A similar pattern of results was observed in Lin and Murray (2014b). These results provide empirical evidence that unconscious effects can increase with stimulus strength. However, far less clear is the exact relation between stimulus strength and the unconscious effect—for example, would it be monotonic (Lin & Murray, in press; Schmidt & Vorberg, 2006; Snodgrass, Bernat, & Shevrin, 2004)?

Therefore, taken together, the nature of unconscious processing should be examined through (at least) two lenses: task sets and stimulus strength. Applying this idea to the question regarding the necessary condition of the unconscious stopping effect, we suggest two clues: (a) the necessary condition would depend on the dissipating speed of the current specific stimulus–stopping task set, which should be modulated by the interference from a new task set; and (b) the dynamics of task set switching would also be modulated by stimulus strength because stimulus strength partly determines the binding strength within the stimulus–stopping task set.

On Demonstrating Unconscious Processing

A main contribution of the awareness priming phenomenon lies in providing clear empirical evidence for, and mechanistic insights into, the consequence of not having equivalent trial sequences between the main and awareness tests (Lin & Murray, 2014a). This principle of equivalent trial sequence is a specific instance of the general notion of avoiding mismatch between the indirect (main) test and the direct (awareness) test when demonstrating unconscious processing (Reingold & Merikle, 1988; Schmidt & Vorberg, 2006). Simple and straightforward, the principle of equivalent trial sequence should be useful when designing experiments to tackle unconscious processing because it helps to ensure that the potential sources of processing that may lead to the indirect effect are not consciously available—a critical requirement when applying the dissociation logic.

However, note that the equivalent trial sequence principle does not curtail the intrinsic interpretation issue in the dissociation logic: the null sensitivity issue in the awareness test. That is, when can we prove chance-level performance? To prove chance-level performance is to prove that no difference exists between an observed performance and the objective chance performance (e.g., 50% accuracy in a two alternative forced choice [2AFC] task), which we can never do—some uncertainty will always exist. Therefore, some threshold must be set to indicate equivalence between the observed and the chance (for an alternative approach, see Rouder, Morey, Speckman, & Pratte, 2007). In the 2AFC example, if the threshold is set at 51%, then the task is reduced to establishing that the confidence interval of the observed performance from the sample completely falls below this predefined threshold. A pressing issue for the research community then is to reach a consensus on the proper threshold to use. Although remarkably little discussion has taken place in the field of unconscious processing, the problem of equivalence has long been discussed in medicine (Alderson, 2004; Altman & Bland, 1995; Berger & Hsu, 1996). Much can be learned from these discussions for setting the proper threshold that is acceptable for establishing equivalence in unconscious processing.

Statistically, the gap between the threshold and the chance is necessarily larger than zero—and the closer the threshold is to the chance, the more trials (subjects) are needed to establish equivalence. Because of statistical uncertainty—inevitable whether one adopts a frequentist approach or a Bayesian one—this gap means that it is possible that the true awareness level is numerically higher than chance. Therefore, one may voice the following concern: Could the observed effect in the indirect task then reflect partial awareness, however small it may be? Because this objection is fundamentally linked to the statistical uncertainty discussed above, it is difficult to rule it out—so difficult, in fact, that some researchers simply seek alternative approaches altogether to unconscious processing (e.g., Reingold & Merikle, 1988; Schmidt & Vorberg, 2006; Snodgrass et al., 2004). Although we always have to live with uncertainty, one way to combat this objection is to (a) supplement the objective awareness test with a trial-by-trial, subjective awareness measure in the main test—hence, a dual task in the main test; and (b) include in the analysis only those trials rated as invisible in the main test and only those subjects who perform around chance in the awareness test (for an example, see Lin & Murray, 2014b, Experiment 2). We suggest that such a *conjunction* method would allow us to make stronger inference regarding unconscious processing.

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